

## ***Interactive comment on “Effects of climate variability on Savannah fire regimes in West Africa” by E. T. N’Datchoh et al.***

**Anonymous Referee #2**

Received and published: 10 October 2012

This paper aims at analyzing fire variability in West Africa at various scales (seasonal, inter-annual, spatial), and to associate these patterns to some climatic drivers (e.g. precipitation, sea surface temperature indices).

This study could potentially be an interesting paper. I believe however that the author should substantially revise the manuscript:

- The introduction should give a better view of fire regimes and fire practices in West Africa, which is critical to understand the modes of variability highlighted in the paper. There is a substantial amount of literature on both these aspects (e.g. P. Laris for fire practices).
- Many of the results are mostly descriptive, with no stated purpose and no discussions

C497

about their findings/implications. That's the case, for example, of the fire statistics aggregated into 4 zones. You don't state the actual outcome of that analysis.

- The discussion section is somewhat detached from the result section and I think would benefit from being rewritten consistently with the objectives of the paper.

As for the structure of the paper, I thought too much text was dedicated to the data/method and results section, at the expense of the discussion. Several figures could also be omitted, or replaced with others:

- Table 1 & 2 look the same to me.
- Figure 4 & 6 too (color scheme only is different). The 1-4 zones in figure 7 could be delineated in figure 4. That would be one figure for figs4,6,7.
- Figure 5a,b: I'm not sure these bring that much more information to the paper.

Below are some additional/more detailed comments following the structure of the manuscript.

### 1. Introduction

ENSO-Fire relationship.

I thought that section could be made more specific to west Africa and the associated literature, and extended to other aspects of climate variability than ENSO (e.g. the other indices you use, NAO, SOI, etc). Additionally, the ITCZ is essential in West African climates and it would be great to discuss it too.

P.1024-I.2: “An El Nino event leads to changes in fire practices.” Please adapt this sentence to reflect the remainder of the paragraph. Human fire practices may adapt to inter-annual climate variability, but you then refer to purely natural drivers (vegetation growth/drying).

### 2. Data and methods

C498

## 2.1 Burned area data

No discussion of data evaluation (e.g. Roy and Boschetti 2009; Chang and Song 2009). Also, no discussion of the implications of a 1km resolution product in West African savannas where fires are very patchy (Laris 2005).

## 2.2. Linear correlation

Not sure that section is necessary. If it is then it should probably be moved after other datasets sections (indices).

## 2.3 MEI

## 2.4 SOI

“the SOI is used to determine the ENSO phase in the pacific”. There is no discussion of how that could be redundant with the MEI.

## 2.5 NAO index

## 2.6 Atlantic North-South temperature gradient

## 3. Results

### 3.1 Intra-annual variability

### 3.2 Inter-annual variability

Confusing paragraph, not sure how the word “trend” is used here (inter-annual) ? “upward trend between 2000-2001 and 2002-2003”, annual burned areas are decreasing over that period. I’m not sure looking for trends is appropriate here, the timeseries is too short (unless there’s a direct impact of a given fire season on the next one, e.g. through fuel reduction or fragmentation).

### 3.3 Spatial variability

Fig. 3 has some interesting insights for other sections of the paper too. Especially

C499

there is indications that 2004-05 was a low fire year because December fires – usually the peak of the season – were largely below-average. What happened that month ?

### 3.4 Frequency of fires

Figure 4 and 6 look quite redundant to me (only the colorbar differs), maybe keep only one ? “The most vulnerable pixels to fire (with a return time under 21 months) represented 16.41% with 2% of these pixels burned every year.” That doesn’t seem to agree with the data in table 1.

### 3.5 Study of main fire source

“The results show that the peak of fire season for zones generally occurred in November”. I think you’re talking about zone 1 only here. Also each zone has a paragraph but zone 3, which is merged to the paragraph of zone 2. It would be clearer for readers if you keep a consistent pattern. Fire source = zone may not be the most appropriate vocabulary (source usually referring to carbon emissions).

### 3.6 Link between burned areas and precipitation

What are the precipitation data you are using ? That should be mentioned in Data/Methods. On figure 10, there is a high correlation between wet season precip and fires, as mentioned in the paper. However there is also a strong correlation, negative this time, with dry season precipitation. You don’t discuss that one.

### 3.7 Effects of large scale climate effects

I’m surprised you get so many indices correlating with so many months in all your 4 zones with coefficients > 0.8. It would be great to have a graphic figure showing time-series of fires and indices to see how they play out. Given the focus on indices given to the paper, it seems essential to me to have that. Also, that section seems to have twice the same result reporting: from p1031,l.22 to p1032,l.20 and from p1032,l.27 to p1033,l.24.

C500

#### 4. Discussion

Overall, the discussion section was quite confusing to me. Most of it discusses general fire ecology concepts (wet season precip and fuel build up, El Nino, etc) without much connection to the results of the paper. I think once you have a clear set of solid results, the discussion should focus on highlighting these, how they complement the existing literature, their impact for our knowledge of regional fire ecology (e.g. for fire modelers, fire practice management).

"Intra-annual variability reveals human activity": not exclusively.

p.1034, l.7: "This evolution showed that the largest areas recorded during the 2000–2001 season were probably favored by the positive anomaly recorded during the previous season (Le Page et al., 2008)." You probably mean the positive anomaly in precipitation from the previous wet season ? The remainder of that paragraph is confusing and the connection with the study results is unclear.

#### References

Chang, D., and Y. Song. 2009. "Comparison of L3JRC and MODIS Global Burned Area Products from 2000 to 2007." *Journal of Geophysical Research* 114 (D16): D16106.

Laris, P. S. 2005. "Spatiotemporal Problems with Detecting and Mapping Mosaic Fire Regimes with Coarse-resolution Satellite Data in Savanna Environments." *Remote Sensing of Environment* 99 (4): 412–424.

Roy, D. P., and L. Boschetti. 2009. "Southern Africa Validation of the MODIS, L3JRC, and GlobCarbon Burned-area Products." *Geoscience and Remote Sensing, IEEE Transactions On* 47 (4): 1032–1044.

---

Interactive comment on *Earth Syst. Dynam. Discuss.*, 3, 1021, 2012.